

bination in which they cannot be recognised by the spectroscope (which is also hardly admissible at that elevated temperature), or that they have been decomposed."

The important work of Victor Meyer on the behaviour of chlorine and iodine at elevated temperatures which must now be regarded as finally established by the experiments of which an account is given in the recent number of the *Berichte der deutschen chemischen Gesellschaft* (March, 1880), and on which I ventured to offer some theoretical considerations in a paper which appeared in the *J. Chem. Soc.* last autumn, point in the same direction. It is difficult not to admit the force of such a body of coincident evidence as that of which I have here given a brief outline.

March 14

B. C. BRODIE

The Aurora at Last

YESTERDAY, Wednesday, March 17, was a magnificently bright sunshiny day from early morning to latest evening; and at 9 p.m. the aurora appeared, just as it used to do years ago in the last sun-spot cycle, and when that strange influence was in its then vigorous existence. Your readers were warned last October that the sun-spots of the new cycle had then begun "in earnest," and now we have to chronicle the first of their auroral fruits.

It was a long low arc of mild quiescent light, about 2° in transverse breadth, 20° long, rising about 7° high in the middle, and sensibly dark between its lower edge and the horizon; the centre was over the north-north-west point of the horizon, but swayed slowly several degrees of azimuth on either side. Towards 11 p.m. the arc began to break up into brighter pellets of light, and these shot fainter rays upwards, making a brilliant and variegated appearance for a few minutes, but apparently soon exhausting itself, for after that there remained only a faint ghostly image of the arc up to 1 a.m.; all this being clearly visible to the naked eye, although a moon seven days old and in 24° N. Decl. was shining brightly in the west.

In the spectroscope, with a narrow slit, nothing but the one inscrutable citron-coloured line appeared, its place in wave-numbers, per British inch, and as ascertained in both hydrogen and carbonic oxide vacuum tubes, being between 45592 and 45690.

PIAZZI SMYTH

15, Royal Terrace, Edinburgh, March 18

A Museum Conference

ALLOW me to deprecate most strongly any attempt to form an association having for its object to talk about museums. Museum officials either know their business or they do not. In the former case they have something better to do than to talk; in the second case the less they say the better. The multiplication of conferences threatens to become a nuisance, and special conferences for every grade, class, and description of humanity will soon be proposed by fussy idlers. We shall be told that it is time to have a conference of housekeepers, of lamplighters, of railway guards, boot-makers, beadles, perhaps ballet-girls.

The fact is that endless time and trouble and money are wasted in England in maintaining rubbishing local museums in the care of ignorant and pretentious curators. Conferences are not required, but *proper salaries* for the curators, who should be educated and capable men; were such men secured by adequate salaries they would soon make the museums in their charge very different from what they now are.

A curator with proper salary ought to be made to attend daily at his museum during office hours, and not allowed to leave it to take care of itself whilst he is lecturing here or there, or eking out his pay by literature.

ACADEMICUS

I AM glad to see that Mr. Paton has, in *NATURE*, vol. xxi, p. 442, again revived the subject of a museum conference, and offers to give his aid towards such attaining a practical form. The desirability of a Museum Association was first suggested in an article in *NATURE*, vol. xv, p. 276, and this was followed by a more definite proposal for a conference by "J. P." and with the addition of a letter in favour of the same object by Dr. Meyer nothing further was published about it. This looked as if the subject was not considered of any great importance, but I believe many curators were decidedly in favour of it, and only awaited it assuming a practical form to give it their hearty support. The success which has attended the Library Association gives every reason to believe that the formation of a similar

association of museum officials would lead to equally good results. Apart from the benefits to be derived from an interchange of ideas and results of experience, which, considering the varied nature of museums and the many practical questions involved in their successful management, could not fail to be considerable, there are many things affecting provincial museums generally that would be greatly advanced by united action. One of these, the distribution of the British Museum duplicates, I should like to refer to. In the British Museum Removals Bill a clause was inserted at the instigation of Mr. Mundella, M.P., and Mr. Chamberlain, M.P., which states that "the Trustees of the British Museum may also give away any duplicate works, objects, or specimens not required for the purposes of the Museum." Instead of giving away, however, I learn that the Trustees are about to *sell by auction* some of the duplicate prints, drawings, &c., in the Museum; and fearing that other duplicates might be disposed of in a similar manner I had the matter brought before Mr. Mundella, who obtained an interview with the Right Hon. Spencer Walpole, Chairman of the Trustees, and was informed that instructions had been given for duplicates in the Natural History Departments to be laid aside and catalogued for distribution among the principal museums. This is somewhat reassuring, but why not treat all their duplicates in this way?—for it should be remembered that provincial towns contribute their share of imperial taxes.

I must not, however, occupy your valuable space with matters that ought properly to be discussed by such a conference as Mr. Paton suggests; and I hope that all who are interested in museums will heartily co-operate with Mr. Paton, whose great success in developing the museum at Glasgow and his extensive acquaintance with museums both in Britain and on the Continent, eminently qualify him to speak with authority on the subject.

Sheffield Public Museum, March 23

E. HOWARTH

A Method of Calculating the Expansion of a Substance on Vaporisation

HAVING occasion last summer to determine the volume of gas which would result from the vaporisation of a given quantity of a certain solid, I made use of a simple way of obtaining approximate results which may not prove uninteresting to some of your readers, unless, as is very likely, they have already made use of it or a better way themselves. For purposes of illustration we may take it that 1 gram of hydrogen gas occupies a volume of 11,200 c.c. at normal pressure and temperature; moreover, the weight of 1 c.c. of water is 1 gm. at 0° C.

Knowing the atomic weight and specific gravity of any liquid or solid, we can now find the volume which 1 c.c. of it will assume on passing into the gaseous state by a simple "rule of three" sum.

For the weight of 1 c.c. of the substance is given by the number which indicates its specific gravity; and the weight of 11,200 c.c. of its gas is given by its atomic weight.

Hence

$$\frac{\text{sp. gr.}}{\text{at. wt.}} \times 11,200 = \text{vol.},$$

to which 1 c.c. of the substance expands on becoming gaseous.

It is interesting to note that the fraction $\frac{\text{sp. gr.}}{\text{at. wt.}}$ is the reciprocal

of $\frac{\text{at. wt.}}{\text{sp. gr.}}$, which is the expression for the so-called atomic

volume (v) of a substance, and thus the expansion of 1 c.c. of a solid or liquid as it becomes gaseous may also be determined by dividing the number 11,200 by the atomic volume of the substance.

In this way we find that 1 c.c. of sodium will occupy 474 c.c. in the state of gas, and 1 c.c. of potassium 249 c.c., or, what is the same thing, 1 c.c. of each of these substances in the gaseous state will occupy $\frac{1}{474}$ c.c. and $\frac{1}{249}$ c.c. in the solid state respectively. But equal volumes of gas under the same conditions of temperature and pressure contain an equal number of molecules, and hence $\frac{1}{474}$ c.c. and $\frac{1}{249}$ c.c., the products of the condensation of equal volumes of potassium and sodium gas must contain an equal number of molecules. From this we readily deduce a real physical meaning for the fraction $\frac{\text{at. wt.}}{\text{sp. gr.}}$, or, in other

words, for the expression "atomic volume," the significance of which, at first, was merely conjectured.

Lists of homologous series of elements and compounds, with their "condensation numbers" attached, give very interesting results; I have only one such at hand, which I here give:—

Butyl series.	At. wt.	Sp. gr.	Condensation.
C ₄ H ₁₀	29	0.600	231.7
C ₅ H ₁₂	36	0.628	192.3
C ₆ H ₁₄	43	0.669	174.2
C ₇ H ₁₆	50	0.699	156.6
C ₈ H ₁₈	57	0.726	142.6
C ₉ H ₂₀	64	0.741	132.9
C ₁₀ H ₂₂	71	0.757	119.4
C ₁₁ H ₂₄	78	0.766	110.0
C ₁₂ H ₂₆	85	0.778	102.5
C ₁₃ H ₂₈	92	0.796	97.0
C ₁₄ H ₃₀	99	0.809	91.5
C ₁₅ H ₃₂	106	0.825	87.1

From this it will be seen that the amount of condensation of the gas, as it passes into the liquid state, becomes less and less as the molecular composition becomes more complex, and moreover, that the difference in amount of condensation of any two adjacent members of the series becomes diminished at the same time; thus the difference in condensation on passing from butyl hydride to amyl hydride is 39.4; from myristyl to benyl only 4.4 c.c. If the fact that the sp. gr. of these substances has been taken at a common temperature, instead of at their boiling points be considered, it will be seen that the difference is really less marked than it otherwise would be.

From the fact that an increase in the number of atoms in the molecules is accompanied by a decrease in condensation, it would appear that a substance might be found which should possess the same, or nearly the same, specific gravity in the state of gas and solid, *i.e.*, in which these states should become identical. A large number of atoms would have to enter into the formation of a molecule to bring about this result, though if there be any truth in the formula for albumin quoted by Herbert Spencer on the authority of Mulder, it must be approached by this substance. This formula— $10(C_{40}H_{121}N_5O_{19}) + S_2P$ —gives us an atomic weight of 3912.5, and if we assume the specific gravity of albumin to be 2, this will give us—

$$\frac{2}{3912} \times 11,200 = 5.8 \text{ about.}$$

So that 1 c.c. of albumin, on this assumption, would be only about five times as heavy in the colloid as in the gaseous state. This fact may help to throw some light on the peculiar properties of colloids, and taken in conjunction with Herbert Spencer's reflections, on that most curious of all colloids:—protoplasm.

W. J. SOLLAS

A Claim for Precedence

IN reference to the notice of Favre in NATURE, vol. xxi. p. 417, I shall be obliged if you will kindly allow me space for a few remarks.

Credit for much valuable work is given justly to Favre, but I must be allowed to protest against having the few grains of corn belonging to others added to his well-filled granary. One of the discoveries ascribed to Favre in your notice is "the relative diminution in the heat evolved by the combination of a compound body compared with that due to the combustion of its varied constituents," or rather, the cause of it. Now he was not the discoverer of this, but I was. In October, 1851, I published in the *Philosophical Magazine* a paper proving that decomposition absorbs heat, and exactly to the same extent that the previous combination of the constituents had evolved it. I proved it by passing a galvanic current through water and finding the increase of temperature. This gave the heat produced by the resistance lessened by the decomposition of the water. Then, to find the heat of resistance undiminished by decomposition, I passed a similar current, as shown by a galvanometer, through a platinum wire offering the same resistance as the water, and surrounded by an equal quantity as in the first experiment. The difference of temperature in the two experiments was, of course, the heat absorbed by the decomposition.

In some twelve months or so after this publication, Favre and Silbermann published in vol. xxxvii. of the *Annales de Chimie et de Physique*, p. 507, the very same experiment to prove the same principle, giving it as their own.

In 1852 a paper from me was read at the Belfast meeting of the British Association, and published the same year, in November, in the *Philosophical Magazine*, giving the first experiments made in thermo-chemistry in which decomposition is taken into account, and showing the principle by which the heat of com-

position of bodies can be known from that absorbed in their decomposition, and which has since been used in all thermo-chemical researches.

This principle and my experiments were published six months afterwards by Favre and Silbermann (*Annales de Chimie et de Physique*, vol. xxxvii. p. 484) without allusion to me.

These publications and dates are easily to be referred to. It is no matter of opinion whether I should be looked on as the originator of thermo-chemistry as it at present stands, but it is a matter of fact that I am, as can be proved by the references I have given; and I think I should not be acting wisely to allow the credit of much labour and thought which is due to myself to be given to another, at least without protest. I do not, of course, want to compare myself with Favre, but I certainly claim, and prior publication establishes for me, the discovery of the principle that originated all the thermo-chemistry of the day which is generally given to him; nor do I expect that I will ever be given as an authority as long as scientific men can quote the names of Favre and Silbermann; but I ask that, in fairness, my claim should be put on record, for although it makes very little difference to the world in general who first works out a discovery, yet to the individual whose only gain is the consciousness of having done it, the matter is quite a different thing.

Parsonstown, Ireland, March 9

THOMAS WOODS

The Origin of Man

SEEING that the doctrine of evolution has gained ground now almost universally among naturalists, it is surprising that the problem of the origin of man does not raise up an army of investigators, resolved to establish it by "demonstrative evidence" on an unassailable foundation.

It is true that this question has been engaging the attention of naturalists, and that individual explorers have devoted themselves to its solution, but little, if any, united effort has been organised hitherto. Were the matter taken up with as much earnestness as has been brought to bear on explorations in Assyria or Palestine, or in the Rocky Mountains of America, it is hard to believe that this question would long remain unsolved.

If the organisation of a society for the purpose of prosecuting research of the kind above indicated were widely agitated, there would not be wanting, I am convinced, either members or funds to further its success. By a strong united effort—international, if necessary—there is no doubt success would be achieved.

We are not utterly without a clue as to the time and place of man's origin. At least such hints as we possess it is our duty to follow up instead of standing by in idle perplexity as to where research should begin.

We now know that it is useless to look for the remains of Simian man in deposits later than pliocene, since the remains of man—not Simian man, but man truly so-called—have been found in pre-glacial and pliocene deposits in the New World as well as in the Old.

The simple fact of the distribution of man over both eastern and western hemispheres having been accomplished as far back as pliocene times, compels us to suppose that man had probably originated not later than the latter part of the miocene age. The discovery of anthropomorpha (*Phopithecus* and *Dryopithecus*) in miocene deposits, while as yet eocene strata have only yielded lower members of the primates seems to point to miocene times as somewhat near the date of man's origination. So much for our present clue as to Time.

Next as to the Place. Inasmuch as the tertiary formations in the New World have produced hitherto no higher members of the primates than inferior types of monkeys and lemurs, except the recently discovered remains of man himself in pliocene deposits, the inquiry may be confined to the Old World.

In the Old World the most northerly position at which the remains of Anthropomorpha have as yet been found is about the forty-fifth parallel of latitude, namely, in Switzerland and in the south of France.

Now as the higher existing apes are only found in Western Tropical Africa and the Malay Archipelago, while the lower apes (the gibbons) extend into Southern China and Northern India. The migration, since miocene times, of the Anthropomorpha, has probably passed along a grand *Main Line* extending from China through Northern India, Balochistan, Persia, Asia Minor into the south and south-west of Europe; and from this main line by *Two Branches*: one from Southern China into Malaysia, the other from the region of the Caucasus through Syria, and perhaps Arabia into Africa.